# ADAPTATION IN THE LIVING AND NON-LIVING[1]

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THE fundamental differences in concept and mode of thought, which may be remarked between the sciences of the living and those of the non-living, are perhaps nowhere better exemplified than in the interpretations and in the degree of prominence which they respectively give to the idea of adaptation. A general survey of the natural sciences results in the somewhat startling discovery that biology is the only one of these which deals conspicuously with this idea. I have, therefore, been led to take, for my present paper, the somewhat bizarre title which has been announced, and I shall here attempt partially to set forth some characteristics and implications of the biological concept of adaptation, and, in certain respects, to compare these characteristics and implications with those of similar concepts which have the place of adaptation in the sciences of the non-living. The term adaptation is used in a passive and in an active sense. I shall consider the two sorts of adaptations in order.

*Passive Adaptations.*—Adaptations are characteristics, properties or qualities attributable to natural objects. They imply, however, not only mere qualities, but also the presence or absence, in the object considered, of *potentialities* or *capabilities* to be or to do certain things under certain conditions. The term always lays stress on potentialities but it does not imply at all that these are, or have been, realized. If they were actually realized, it would amount to a redundancy to note the existence of the adaptations at all; an adaptation "caught in the act," an already realized potentiality, is so self-evident that we do not need to mention it as such. In such a

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case, the statement of the realization implies the potentiality, for an object is obviously adapted to doing and being what it does and is.

However, doing and being are only relative, for any object may change its state more or less effectively and may possess attributes in different degrees, and our interest in realized potentialities lies not in the *fact,* but in the *degree* of adaptation. Furthermore, the degree of adaptation depends clearly upon the extent to which the object considered possesses those properties or qualities about which our idea of adaptation centers, and so attention is at once turned to the properties or qualities as such.

I may draw an illustration of actually observed adaptations from the science of geology, which perhaps interests itself as much in the survival and distribution of rock masses as does biology in the survival and distribution of living things. My example has to do with the natural selection and distribution of certain boulders and pebbles in a deep Californian valley.

At the time of the filling of the Salton sink by the unruly Colorado River, the only loose stones of the inundated area that were able to keep themselves in contact with the air were fragments of pumice. These were adapted to float upon water, and they largely refused to be submerged. With the rising waters they also rose, and thus were able to take advantage of air movements to redistribute themselves. Had it not been for the floating adaptation, these pumice pebbles would have suffered temporary extinction in the form of submergence, and they would not have been able to survive and to gain dominance in the pebble population of certain Salton beaches which they forthwith proceeded to invade. We have reason to believe that this sort of spontaneous migration of pumice pebbles has taken place many times before in the Salton valley, at periods and seasons when edaphic and climatic conditions happened to favor such readjustments of the tension lines, and that the present

distribution of these curiously endowed stones has been largely brought about through their possession of the floating adaptation.

Probably a geologist would not mention adaptation in this connection; he might succinctly state the apparent specific gravity of pumice, and might then proceed to present the case in terms of this internal character and in terms of more or less quantitatively known features of the surroundings; for the idea of adaptation here takes account of the low specific density of the rock and the ability to float upon water implied thereby, and to state that pumice is adapted to float is redundant, after we know its specific density. For aught I know also, there may be different species of pumice, some of which might be observed to float higher or a longer time upon water, and in such case, what we might term the varying degrees of adaptation in the different species should be quantitatively brought out—and then dismissed—by an adequate study of the internal qualities of the rocks.

But not nearly all of the potentialities discoverable in natural objects are of this realized, and consequently directly observable sort. The future is no doubt pregnant with hitherto untested adaptations and our imagination frequently suggests these as problems. Of course modern natural science responds to the proposal of this sort of adaptation, *try it and see if it is true,* and many of us are busy just now in doing this very thing.

If we find observational proof of the suggested property, interest in its adaptational aspect fades, for the case then passes over into my first category, of actually observed adaptations. Also, the experimental test of a proposed potentiality—as to whether it is attributable or not to the object considered—is but a case of observation, properly prepared for. Not readily finding the necessary conditions and the object together in nature, we find them separately (at different places or times) and proceed to bring them together. Thus my first experience of the Salton valley was had at a time when it contained

little or no visible water. Let us suppose that interest was at that time aroused in the distribution of pumice pebbles upon certain areas of the dry valley floor, and let us suppose that a previous migration of these, similar to the one just described, was suggested as a possibility. At that time the direct test by observation was not possible, but the whole question—as far as the floating adaptation is concerned—could have been settled readily enough either by bringing water to the pebbles or by taking some of the pebbles to water. But of course we should have been dealing, in this instance again, with the determination of the presence or absence of a certain property in the pebbles, as related to a certain property of water.

In the vast majority of the cases of this sort that attract our attention at the present time, however, natural science is unable to obtain direct observational tests, even of the experimental sort, and some indirect method of comparison must be resorted to. Now, indirect methods for determining the degree of a proposed adaptational property consists in nothing more than the determination, by whatever means may be convenient, of the degree to which this property exists in the object considered. Thus, without ever bringing water and pumice together, it is perfectly possible to establish the ability of the latter to float, as by determining the comparative weights of equal volumes of the two substances.

From what has preceded it is suggested that every passive adaptation that we may consider resolves itself, upon adequate analysis, into a problem of the measurement and quantitative comparison of qualities or properties of objects. If neither the direct experimental test nor the requisite measurements can be carried out, then the suggestion of an adaptation is no different from the statement of any other problem for which no method of attack has yet been devised. But it must be recognized in this connection that the usual biological adaptation is not always appreciated as a problem in comparative

measurement, and that its proposal, especially if made by one in an authoritative position, is far too apt to be received as a declaration rather than as a question. Thus, for example, our elementary texts may tell the innocent beginner that brightly colored flowers are better adapted to fertilization by insects than are their less gaudy neighbors, and without critical analysis, a very complex and exceedingly difficult problem is at once regarded as solved. As a matter of fact, this problem involves comparative measurements for which methods have not yet been devised, so that the cautious biologist must regard the question of this proposed adaptation as utterly beyond us for the present.

Apparently possible potentialities which have not been actually observed in nature, or which have not a basis in quantitative comparisons so as to be possible of definite establishment or refutation, have not played an important rôle in the modern development of the sciences of the non-living, and consequently the adaptational aspect of the qualities of natural objects is seldom mentioned in these sciences. The relative ease with which the qualities of the non-living may now be analyzed into fundamental concepts renders the use of any other terms than those of matter and energy quite out of place in their serious discussions. On the other hand, biological inquiry has still much to do with theoretical attributes which can not be put to any satisfactory test, and this condition may be in part responsible for the prevalence of the adaptational point of view in our science. It seems to be partly because biological problems are too complex for ready analysis at present, that the adaptational properties of living things are so often stated in terms other than those of the fundamental concepts of matter and energy.

In this connection it is, however, to be remembered that ease of analysis depends as much upon the state of the analyzing mind as upon the complexity of the analyzed object. A mind is conceivable, I think, that would con-

sider the phenomenon of cell division as just as capable of analysis as is that of a chemical reaction like flame; and we are sure that there have been in the past (and are indeed at present) minds to which flame would appear quite as hopeless of analysis as does cell division to us. I have said that the qualities of living things are too complex for analysis at present, I might as well have said that we are at present too ignorant and too feeble to analyze such qualities. Our science is young yet, not in years, perhaps, nor yet in absolute achievement, but in the relation of its present phase of development to the task which is set before it. It appears to be this youthful quality in biology which may partially explain, as I have said, the somewhat startling generalization with which we began.

*Active Adaptations.*—Biological writing employs the term adaptation in an active sense as well as in the passive one heretofore considered in this paper, and it remains to give some attention to this usage, and to an apparent confusion of cause and effect which is connected therewith. To obtain a clearly legitimate case of active adaptation we shall have to turn to human affairs, for reasons which will soon be evident, and the familiar adaptation of the watch will serve our need as well as any other. The little machine is complex, too complex for most of us to understand, and it seems to be much better adapted than any living thing to long-continued, uniform motion of a certain specified kind. If I make inquiry regarding the causes, or antecedent conditions, to which this adaptation is due, I find that the various parts have been made and assembled with reference to the very adaptation which I am investigating. In my search after causal relations I have been entrapped in a mesh of uninvestigated psychological phenomena and have discovered the puzzling truth that the watch is what it is, simply because *it was to be what it is!* In other words, the cause of the effect which we are considering is regarded as some sort of disembodied spirit of the effect

itself, and this effect, in order to be the cause of itself must have existed before it came into being.

Of course we realize that we have thus come into contact with the darkest problem with which biological science has to deal, namely, the problem of human purposeful action and of the human will. While I see no reason for doubting that this problem may eventually yield to analysis and comparative measurements, yet it must be admitted that progress in this direction has only just begun, so that anything but the most superficial considerations in this connection is at present but waste of time and trouble. We have here, for the present, to acknowledge our fundamental ignorance, and to hold our minds in that state of suspended judgment to which, in less complex affairs, students of all the natural sciences have become used.

Although we are as yet unable to analyze into simple terms of matter and energy the antecedents which conditioned the adaptation that is before us in the watch, yet it seems that our analysis of the universe about us has progressed far enough so that we may be justified in frankly maintaining that the problem of purposeful causation has no place in any of our considerations, excepting solely those wherein human consciousness has been involved among the causal conditions. To employ other terms, I think we are bound to regard the nature of the future outcome of all processes as totally non-existent, and consequently absolutely without influence in the present, excepting alone (and temporarily) those processes for which the human will is accounted a necessary antecedent.

We may now inquire as to the causes which have been in operation to bring about the peculiarly low specific gravity of the pumice pebbles in my case of the floating adaptation of these bodies. We are assuredly unable to state these causal conditions in anything approaching completeness, but we are nevertheless sure that human purposefulness has played absolutely no part in the

matter, so that we put the floating adaptation of these pebbles entirely out of mind as soon as we begin our search for the causes thereof.

Geology, it seems, does not find purposeful action necessary in its explanations. Neither does any progress come from a consideration of purposeful changes in the readjustments of atoms with which chemistry deals, nor in the phenomena of heat migration which constitute a portion of the field of physics. Modern astronomy sees no purposeful activity in the motions of sidereal space, nor does meteorology seek in the effects of a storm any suggestion of the causes which brought it into being. Many branches of biological science, however, although confessedly not dealing with human purposeful activity, seem frequently to seek in the future the causes of the present. Thus our science teems with purposeful reactions, and this feature of the idea of adaptation adds its influence to the ones already mentioned, playing an important rôle in keeping the sciences of the non-living essentially and fundamentally separate from those of the living.

The history of the idea of causation in the natural sciences suggests much that may have a bearing on our judgment as to whether this present distinction between the two groups of sciences is necessary and permanent or merely temporary and passing. To primitive man all problems were too complex for adequate analysis, and purposeful activities of many kinds were devised to explain not only the doings of his fellow men but also the doings of all nature. The whole world was then a world of teleological causation. The heavenly bodies moved and the chemical elements combined or separated according to the capricious wills of innumerable deities and demons. Men then heard in the howling of the storm and in the rumble of the earthquake the terrible voices of the spirits of the air and earth. All living things were endowed with a man-like consciousness and power of willing to do, and everything struggled with everything else in a never-ending conflict of capricious wills.

In the development of our race, however, an increasing experience of deterministic causal relations has been accompanied by a progressive effort to expunge the idea of purposefulness from our thinking. The numerous mythological personages just called to mind have gradually suffered a curtailing of their powers for good and evil, and have, in general, by natural science at least, been totally discarded. Many relics of the past of course remain in all our mental life; many of our words and not a few of our instinctive modes of thought are survivals from the teleological period of our development. Jupiter and Venus still play their part in modern astronomy, and Vulcan's name is still heard among geologists. But obvious teleological expressions have been generally outgrown and discarded by all of the sciences that deal with the non-living. In biology alone they persist, mainly as personifications of plants and animals, making our modern writings a curious jumble of exactly stated observations and conclusions, together with many statements that might have been taken bodily from primitive fairy-tales. Foreseeing of the future and conscious purpose are apparently attributed to living things in which we have no evidence for the existence of consciousness. The eye develops in the animal *in order that* it may see, the leaves of the plant are *for the purpose* of obtaining carbon dioxid from the atmosphere. The list of such statements might be made very long, but you are quite familiar with their nature.

Not only are the organisms with which we deal frequently personified to the extent of attributing to them the foresight and will needed to carry out complicated plans, but they are also frequently supposed to be capable of making *a mistaken judgment.* I find in Gibson's translation of Jost's "Plant Physiology," 1907 (page 389), an excellent example of this assumption, where it appears that one lower organism may be clever enough to outwit another. The statement in question reads, "The gall, for example, is of service only to the insect,

but is highly disadvantageous to the plant; we must assume, indeed, by way of explanation, that the insect succeeded in deluding the plant, so that instead of treating the insect as an enemy and an intruder it behaved towards it as if it were a bit of itself."

I think it is perfectly clear that the non-biological sciences have all passed through a much earlier stage in which purposeful adaptations were seriously considered, and it seems quite as clear that such concepts are not any longer accepted in the serious studies of these sciences. There seems also to be no doubt that the biological sciences, notably in their physiological aspects, are tending at the present time more and more to adopt a non-teleological point of view. From these points I again draw the conclusion that ours is a developmentally young science, that it still retains features of its early youth, and that the concept of purposeful adaptation is one of these features, sooner or later to be totally abandoned, even as the same concept has already been abandoned by the other natural sciences.

If my conclusion should be wrong, then one of two propositions must follow: either the sciences of the non-living have fallen into error, ought to have retained the concept of purpose in natural phenomena, and will sooner or later return to this concept; or else there is a great and fundamental difference between the living and the non-living, and teleology has a logical place in considerations of the former objects but not in those of the latter. Although it is to be realized that the possibility of one or the other of these propositions can not be rigidly denied at the present time, yet the probability of either one is definitely decreased by every analytical conquest of science. The controversy here suggested—which seems in our time as wastefully to absorb our energies as did the discussion of special creation in the time of Charles Darwin—is characterized by this peculiar feature, that, while all evidence presented *for* teleological causes is conspicuously based upon our ignorance and present inability to

analyze our problems, the evidence offered in the opposite direction is just as conspicuously positive and consists of cases which have already been subjected to relatively complete analysis. As Cowles[1] has pointed out, there has never been any retrogression in these matters; *all* phenomena now explained non-teleolgically were once explained teleologically, but no non-teleological explanation once attained, has ever been replaced by one involving purpose. Under these circumstances, a pragmatic judgment must be rendered, at least tentatively, in favor of the position here taken, that teleological thinking should have, and will at length have, no place in our science at all.

*Conclusion.*—I think it is to be concluded from the considerations here set forth that there is nothing known of the nature of living things which should lead the biological sciences to base their inquiries on any other methods or modes of thought than those employed in the sciences of the non-living. In both its aspects, passive and active, the dominance of the concept of adaptation, which now distinguishes our science from the non-biological ones, is related to the comparatively youthful stage of development so far attained by biology, and not to any observed character of the living objects with which we deal. It seems obvious that biology is advancing slowly but steadily along the path already traversed by the other natural sciences, and I think our present operations may best be guided by the hypothesis that all these sciences will eventually come to deal with the same fundamental concepts and modes of thought. Should this condition of affairs ever come to actual attainment, then the discussions which now center about the idea of adaptation might be expected to give place to other discussions of causal relations between measured qualities and properties of the objects dealt with, such as are already beginning to be common in many lines of biological study.

[1] Cowles, H. C., in Coulter, Barnes and Cowles, "Text-book of Botany," 2: 948. 1911.